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Employment Under Marijuana

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Abstract

This study examines the impact of recreational marijuana laws (RMLs) on firm-level employment using an imputation-based difference-in-differences (DiD) approach across U.S. states. RMLs significantly reduce employment, particularly among firms with high-skilled labor, strong union presence, permissive corporate cultures, and in states with greater dispensary density. Alternative explanations—including economic crises, COVID-19, fiscal changes, labor regulations, and related policies such as smoking bans and right-to-work (RTW) laws—are systematically ruled out through a series of placebo and robustness tests. RMLs also reduce investment, sales growth, and innovation, suggesting that legalization introduces labor-related frictions with broad implications for firm performance and long-term dynamism.

I. Introduction

> The best way I could describe the effect of the marijuana and hashish is that it would make me relaxed and creative.[1] (Steve Jobs)

> I'm not a regular smoker of weed… I don't find that it is very good for productivity.[2] (Elon Musk)

These contrasting statements capture the persistent ambiguity surrounding the cognitive and behavioral consequences of marijuana use—ambiguity that is increasingly salient in light of ongoing policy liberalization across the United States. While public interest and macroeconomic analysis of marijuana policy have grown, the firm-level implications remain underexplored in the finance literature.

Existing empirical research offers valuable but incomplete insights. Much of it has focused on medical marijuana laws (MMLs), which apply to a narrower population and typically exert limited influence on adult-use prevalence (Cerdá, Mauro, Hamilton, Levy, Santaella-Tenorio, Hasin, Wall, Keyes, and Martins (2020)). In these contexts, access to marijuana has been shown to improve labor

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[1]"Steve Jobs' Pentagon papers: blackmail fears and taking LSD," Ryan Faughnder, *Los Angeles Times* (June 11, 2012). https://www.latimes.com/business/la-xpm-2012-jun-11-la-fi-steve-jobs-pentagon-file-20120611-story.html.

[2]Interview by Joe Rogan, The Joe Rogan Experience, Episode 1169 (2018).

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market outcomes by alleviating health-related employment constraints (Blake, Robson, Ho, Jubb, and McCabe (2006), Fiz, Durán, Capellà, Carbonell, and Farré (2011)). In contrast, recreational marijuana laws (RMLs) extend access to a broader and generally healthier working-age population, raising distinct behavioral and organizational concerns for firms. Several studies have examined the effects of RMLs on individual- or state-level labor market outcomes, often reporting null or modestly positive effects on employment, wages, or participation (Morris, TenEyck, Barnes, and Kovandzic (2014), Chakraborty, Doremus, and Stith (2021), and Dave, Liang, Muratori, and Sabia (2025)). However, these findings are frequently drawn from early-adopting states or narrow industry contexts, limiting their generalizability. More importantly, the literature rarely considers how firms themselves, rather than workers, respond to legalization. In this context, understanding how expanded legal access to marijuana affects corporate labor demand, investment strategy, and innovation is essential for both academic inquiry and policy design.

The legalization of recreational marijuana represents an institutional shift with complex implications for firms. On the one hand, proponents argue that RMLs stimulate economic activity by creating new jobs, expanding retail and agricultural sectors, increasing tax revenues, and reducing opioid-related harms (Bradford and Bradford (2018), Chakraborty et al. (2021)). They may also reduce criminal convictions and improve employment prospects for historically marginalized groups (Sabia and Nguyen (2018)). These mechanisms may indirectly benefit firms by expanding the local labor supply, reducing hiring frictions, and improving access to marginalized talent pools. On the other hand, critics raise concerns that increased access may impair cognition, raise absenteeism, and lower productivity, particularly in cognitively demanding or safety-sensitive roles (Anderson and Rees (2014), Volkow, Baler, Compton, and Weiss (2014)). Shifting workplace norms could further challenge coordination and managerial control (Anderson, Hansen, and Rees (2015), Albino (2017)).

This article addresses this gap by exploiting the staggered adoption of RMLs across U.S. states and implementing a difference-in-differences (DiD) framework to estimate causal impacts on firm-level employment. Recognizing the limitations of traditional two-way fixed effects (TWFE) models—particularly in the presence of heterogeneous treatment effects—I apply the imputation-based DiD estimator developed by Borusyak, Jaravel, and Spiess (2024). This method constructs counterfactual outcomes using untreated firms, weighted by treatment timing and observable characteristics, to estimate the average treatment effect on the treated (ATT). The panel spans 2007–2023, covering pre- and post-legalization periods, and incorporates firm- and year-fixed effects, along with a rich set of firm- and state-level controls. To better capture geographic exposure to RMLs, this article also examines a subsample of small firms (fewer than 500 employees), which are more likely to operate within a limited regional footprint and thus be more directly affected by state-level policy changes.

The empirical findings reveal that RMLs significantly reduce firm-level employment, with the most pronounced effects emerging 3 to 4 years post-legalization. Employment trends for treated and control firms are statistically indistinguishable prior to adoption, supporting the parallel trends assumption.

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Economically, RMLs reduce firms' employment growth by at least 2.7 percentage points. The results are robust to various model specifications and persist after controlling for macroeconomic conditions, the COVID-19 pandemic, and contemporaneous policy reforms.

This analysis further documents considerable heterogeneity in the impact of RMLs across firm and state characteristics. Firms with a higher proportion of high-skilled workers experience more substantial employment declines, consistent with heightened vulnerability in cognitively demanding roles. Effects are also more pronounced in unionized industries, possibly reflecting institutional rigidities that limit firms' ability to respond to productivity shocks. Additionally, firms located in states with greater dispensary density (used as a proxy for market penetration) exhibit sharper employment reductions. Organizational culture also appears to moderate RML effects: Firms scoring highly on "respect" in culture indices, which may indicate more permissive or autonomy-oriented environments, suffer greater employment losses. These patterns suggest that internal workplace norms condition how external behavioral policies are absorbed.

In light of these findings, this article extends the analysis to explore whether RMLs influence broader dimensions of firm behavior beyond employment. Statistically significant declines are documented in capital investment, R&D expenditure, sales growth, and labor productivity following RML adoption. Consistent with the heterogeneity results—particularly those emphasizing the heightened sensitivity of high-skilled firms—innovation outcomes also deteriorate. Treated firms file fewer patents, particularly exploitative ones, receive fewer forward citations and demonstrate lower overall patent quality. These findings suggest that RMLs have wide-ranging implications, affecting not only firms' employment levels but also their long-term growth trajectories and innovative capacities. However, no statistically significant negative effect is found on Tobin's Q.

To further reinforce the credibility and specificity of the main findings, this article implements a series of placebo tests and robustness checks. First, a permutation-based falsification test is conducted by randomly assigning treatment years to RML-adopting states and reestimating the ATT 500 times. The distribution of these placebo estimates centers around zero, with none as negative as the actual treatment effect. This suggests that the observed employment effects are unlikely to be driven by chance or unrelated time trends.

In addition, this analysis assesses whether the heterogeneity results are unique to RMLs rather than artifacts of model specification. To this end, the triple differences (DDD) heterogeneity framework is replicated using placebo policies unrelated to marijuana, namely Control Share Acquisition (CSA) laws and Business Combination (BC) laws. While these corporate governance laws may influence firm behavior in other domains, they are not expected to impact employment through RML-specific channels such as marijuana exposure, unionization, or skill intensity. Using pseudo-treatment years and the same DDD specification applied in the main heterogeneity analysis, none of the placebo interaction terms are found to be statistically significant. These tests, alongside other robustness checks, including alternative sample restrictions, controls for legal confounders, and alternative operationalizations of RML exposure, collectively strengthen the conclusion that

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the employment and heterogeneity effects observed are specific to recreational marijuana legalization.

This article makes three key contributions to the literature on recreational marijuana legalization and corporate finance. First, this study examines the firm-level employment effects of RML, offering a new perspective distinct from the dominant focus in prior work on public health, social equity, or tax revenues[3] (e.g., Jacobi and Sovinsky (2016), Bradford and Bradford (2018), Brinkman and Mok-Lamme (2019), Hansen, Miller, and Weber (2020), Mace, Patel, and Seegert (2020), Hollenbeck and Uetake (2021), Sabia, Dave, Alotaibi, and Rees (2021), (2024), Auriol, Mesnard, and Perrault (2023), Cheng, De Franco, and Lin (2023), Mathur and Ruhm (2023), Dave et al. (2025), and Guernsey, Serfling, and Yan (2025)). While several studies have explored the aggregate labor market or state-level effects of marijuana laws (e.g., Anderson and Rees (2014), Morris et al. (2014), Dave et al. (2025)), this article focuses on the firm as the unit of decision-making, where hiring, investment, and employment structures are determined.

In particular, this analysis complements recent finance-oriented work on marijuana legalization. Guernsey et al. (2025), for instance, show that MMLs decrease firms' cost of equity and thus reduce labor market risk, while Cheng et al. (2023) show that the passage of MMLs is associated with higher state bond spreads. These studies highlight the informational and market-pricing consequences of marijuana liberalization. In contrast, this study focuses on firms' internal, real-side adjustments—particularly those involving human capital and innovation. By isolating the effect of RMLs on corporate hiring, especially in firms with high exposure to marijuana-related frictions, this article provides novel evidence on how behavioral policy reforms manifest as organizational frictions. Crucially, the findings suggest that the consequences of marijuana legalization extend beyond the marijuana industry, highlighting significant spillover effects on labor, innovation, and productivity across a wide range of firms.

Second, this study contributes to the growing literature on labor market frictions and firm behavior, which examines the interplay between labor market frictions and firm performance (e.g., Atanassov and Kim (2009), Boustanifar (2014), Belo, Lin, and Bazdresch (2014), Donangelo (2014), Giroud and Mueller (2015), Belo, Li, Lin, and Zhao (2017), Kuehn, Simutin, and Wang (2017), Donangelo, Gourio, Kehrig, and Palacios (2019), Benmelech, Bergman, and Seru (2021), and Nishesh, Ouimet, and Simintzi (2024)), by identifying a new, policy-driven source of friction: the legalization of recreational substances. The findings show that RMLs induce frictions in workforce management, particularly in firms with high-skilled labor, strong union presence, or tolerant corporate cultures. These frictions manifest in reduced employment, lower productivity, and ultimately dampened firm investment and innovation. In this way, this article extends recent work on the financial consequences of health-related societal shocks, such as the opioid crisis. Cornaggia, Hund, Nguyen, and Ye (2022) demonstrate that opioid misuse imposes fiscal costs via increased mortality and social disruption, Ouimet, Simintzi, and Ye (2025) observe a negative effect of opioid prescriptions on

[3]Interested readers may refer to Anderson and Rees (2023) regarding the effect of marijuana legalization on public health.

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subsequent individual employment, and Jansen (2023) shows that the opioid epidemic generates negative spillovers in household credit and consumption through increases in loan defaults. This study complements this literature by showing that even state-sanctioned behavioral liberalization—rather than public health deterioration—can have unintended adverse effects on firms.

Third, this article contributes to the broader literature linking legal institutions and behavioral regulation to corporate outcomes. While much of the existing research—including seminal works by La Porta, Lopez-de-Silanes, Shleifer, and Vishny (1998), Bertrand and Mullainathan (2003), Bhattacharya and Daouk (2009), Francis, Hasan, John, and Waisman (2010), Serfling (2016), Karpoff and Wittry (2018), Chava, Danis, and Hsu (2020), Frattaroli (2020), Gao, Hsu, Li, and Zhang (2020), and Olson, Yust, and Christensen (2023)—examines how legal institutions and regulatory constraints shape firm behavior, this study focuses on the economic consequences of marijuana legalization. Building on this body of research, this analysis examines how such legal changes affect both innovation outcomes and employment dynamics.

In contrast to Gao et al. (2020) and Olson et al. (2023), who find that smoking bans enhance innovation by modifying employee behavior and workplace dynamics, this article investigates the effects of relaxing behavioral constraints. Despite the opposing directions of these interventions—restriction versus liberalization—this analysis finds comparable magnitudes of effect. Importantly, this study builds on these prior studies by explicitly controlling for the presence of smoking bans at the state level. This allows the analysis to isolate the effect of marijuana legalization from other cooccurring public health policies. Even after accounting for these existing restrictions, this article finds that firms exposed to RMLs experience significant declines in both exploitative and exploratory patenting, forward citations, and overall patent quality, alongside adverse shifts in employment dynamics. The findings suggest that liberalization can be as impactful as restriction, not only in shaping innovation outcomes but also in influencing workforce structure and labor market responses—albeit with potentially negative consequences. This suggests that RMLs introduce a distinct channel of behavioral friction—possibly related to changes in cognitive performance, absenteeism, or workplace norms—that extends beyond the mechanisms identified in the smoking ban literature. In this way, this article broadens the scope of the law-and-finance literature by demonstrating that socially motivated, noncorporate policies can have substantial spillover effects on firm operations and strategic decision-making. These results underscore the importance of incorporating the wider institutional and behavioral environment into corporate finance research.

The structure of the article is organized as follows: Section II introduces the institutional background of marijuana legalization, Section III outlines the identification strategy and data, Section IV presents the main empirical findings, and Section V concludes.

II. Institutional Background

The legal status of marijuana in the United States has undergone substantial transformation over the past century, marked by shifting regulatory frameworks at both the federal and state levels. In the late 19th century, industrial hemp was widely

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cultivated—particularly in states such as Kentucky—playing a significant role in the agricultural economy (United States Bureau of the Census (1913)). However, the early 20th century saw growing societal and political opposition to marijuana, resulting in the introduction of state-level prohibitions during the 1910s. These efforts culminated in federal restrictions through key legislative interventions, including the Uniform State Narcotic Drug Act of 1935 and the Marihuana Tax Act of 1937. The latter imposed taxation and stringent regulatory controls on the importation, cultivation, possession, and distribution of marijuana, with violations subject to fines and imprisonment (Sabia and Nguyen (2018)).

A temporary revival in hemp production occurred during the Second World War, driven by material shortages that necessitated increased domestic sourcing (Johnson (2014), Hudak (2018)). Nevertheless, following the war, marijuana remained tightly regulated. In 1970, federal prohibition was further reinforced by the Controlled Substances Act, which classified marijuana as a Schedule I substance—indicating a high potential for abuse and no recognized medical application—thereby rendering it illegal under federal law nationwide.

Despite this enduring federal stance, state-level challenges to prohibition gradually emerged. California led this shift in 1996 by enacting Proposition 215—the Compassionate Use Act—which legalized medical marijuana (National Conference of State Legislatures (2020)). This landmark legislation catalyzed further state-level reforms and signaled a pivotal change in policy direction. The shift was further solidified by the U.S. Department of Justice's 2009 memorandum, which advised federal prosecutors to deprioritize enforcement against individuals and businesses operating in compliance with state MML. This federal guidance contributed to increased legitimacy and investment in the nascent marijuana industry (Stout and Moore (2009)).

The momentum toward legalization accelerated markedly in 2012, when Colorado and Washington became the first states to approve recreational marijuana use through ballot initiatives. Since then, the legal marijuana market has expanded rapidly. By the end of 2023, 39 states and the District of Columbia had legalized medical marijuana, while 24 of those states—plus the District—had also legalized recreational use (as illustrated in Figure 1). The timelines for implementing recreational sales vary widely across jurisdictions, ranging from under 3 months in Arizona to up to 4 years in Maine. On average, states experienced an implementation lag of approximately 16 months (Ward (2022)).

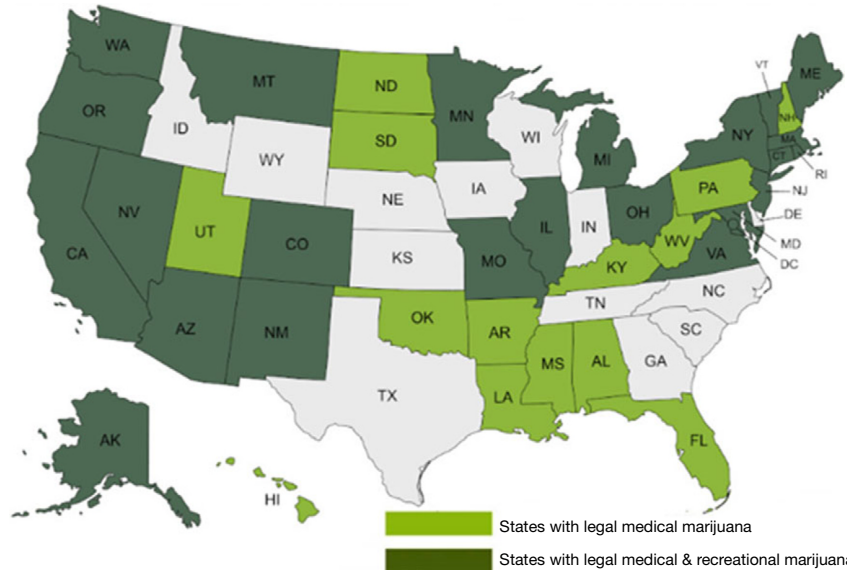
In parallel, numerous states have adopted partial reforms, including the decriminalization of small-scale possession and the legalization of low-Δ^9-tetrahydrocannabinol (THC) cannabidiol (CBD) for medical purposes. These legal changes reflect a broader societal shift in attitudes toward marijuana. According to recent surveys, a record high 68% of Americans now support legalization—up from approximately 50% a decade earlier (Gallup (2025)).[4] This shift in public sentiment suggests continued momentum for reform, with many states actively considering new legalization or ballot measures concerning both medical and recreational marijuana use.

[4]https://news.gallup.com/poll/284135/percentage-americans-smoke-marijuana.aspx.

FIGURE 1

State-by-State Recreational Marijuana Laws

Figure 1 presents a choropleth map of the United States illustrating the legal status of marijuana at the state level. States are categorized into two groups: i) states with legalized medical marijuana only (depicted in light green), and ii) states with both legalized medical and recreational marijuana (depicted in dark green). States shown in white/grey indicate jurisdictions where marijuana remains prohibited in both forms. Source: ProCon.org at https://marijuana.procon.org/legal-recreational-marijuana-states-and-dc/.



III. Data and Empirical Approach

A. Empirical Approach

I exploit the staggered adoption of recreational marijuana legalization across U.S. states within a DiD framework, initially employing a TWFE specification. The key identification assumption is that firms located in states yet to legalize recreational marijuana serve as valid counterfactuals for treated firms, conditional on observable and time-invariant unobserved factors.

The baseline specification is defined as follows:

$$(1)\quad Employment_{i,s,t} = \begin{aligned}&\alpha + \beta_1 Treat_s \times Post_t + \beta_2 Post_t + \beta_3 Treat_s + \rho_1 X_{i,s,t} \\ &+ \rho_2 Z_{s,t} + \varphi_i + \vartheta_t + \varepsilon_{i,s,t},\end{aligned}$$

where i denotes the firm, s the state, and t the year. $Employment_{i,s,t}$ is defined as the annual change in the number of employees for firm i in state s at year t. $Treat_s$ equals to 1 when a firm is headquartered in a state that has legalized recreational marijuana by year t, and 0 otherwise. $Post_t$ corresponds to a dummy that is set to 1 for years after the state legalized recreational marijuana, and to 0 otherwise. β_1 indicates primary interest in this regression. If marijuana legalization matters for a firm's employment, then β_1 should be significantly different from 0. I cluster standard errors by state to account for serial correlation in the standard errors within a firm over time and across firms within the same state.

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$X_{i,s,t}$ represents a set of control variables that account for firm-specific characteristics. Building on established literature (e.g., Ellul, Pagano, and Schivardi (2018), Ersahin and Irani (2020), Benmelech et al. (2021), and Peng, Shi, and Yu (2025)), I incorporate controls for key firm-level variables such as leverage, Tobin's Q, cash flow, size, return on assets (ROA), capital investment (CAPX/AT), and sales growth. $Z_{s,t}$ represents state-level factors, including the gross state product (GSP) growth rate, unemployment rate, government spending-to-GSP ratio, taxation freedom, labor market freedom, and state-level economic policy uncertainty (EPU), capturing broader macroeconomic conditions. I also consider whether the headquarters state is a blue state, whether it is incorporated in Delaware, whether it bans smoking in public places, and whether it adopts right-to-work (RTW) laws, as each of these factors may influence corporate decisions (e.g., Daines (2001), Chava et al. (2020), and Gao et al. (2020)). Each of these policy controls is specified as an indicator variable that equals 0 prior to the policy's enactment, and 1 in all subsequent years. Moreover, I include firm-fixed effects (φ_i) to capture the time-invariant firm-specific characteristics, while year-fixed effects (ϑ_t) reflect the unobserved heterogeneity that varies across time.

A key consideration in the main analysis is the geographic dispersion of a firm's workforce. For example, although Starbucks is headquartered in Seattle, WA, most of its employees work outside Washington. In such cases, identifying the "relevant" location that influences a firm's employment decisions following regulatory changes can be challenging, potentially affecting the estimates. To address this concern, rather than relying solely on the full sample, I also focus on smaller firms that are more likely to have the majority of their workforce concentrated at their headquarters. This approach helps ensure that the main identification strategy more accurately reflects the intended geographic dimension of treatment.

Regarding the estimation method, TWFE models can introduce biases in staggered DiD frameworks due to assumptions about homogeneous treatment effects and variations in treatment timing. These biases emerge when treatment effects evolve over time or when units treated later serve as controls for those treated earlier, potentially leading to distorted results (Callaway and Sant'Anna (2021), Goodman-Bacon (2021), Baker, Larcker, and Wang (2022a), and Borusyak et al. (2024)). To mitigate these issues, I employ the imputation-based approach proposed by Borusyak et al. (2024), which effectively addresses heterogeneity in treatment effects and the timing of treatment.

This imputation-based method offers several advantages: It is transparent, exhibits favorable efficiency properties, and allows for the estimation of asymptotically conservative standard errors. The method enhances the precision of causal estimates by constructing a counterfactual employment trajectory for firms not exposed to marijuana legalization, using the control group to predict employment dynamics in the absence of RML. It accounts for both firm-specific and time-specific factors and then imputes what the employment levels would have been for treated firms during the post-legalization period, assuming they had not been affected by RML adoption. The causal effect of RML is estimated by comparing the

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imputed employment levels with the observed levels in the treated group. These estimates are subsequently aggregated and weighted to calculate the average treatment effect.

B. Data and Summary Statistics

The primary sample comprises data spanning from 2007 to 2023. I initiated the analysis in 2007 to establish pre-RML trends before the first RML laws were enacted by Washington and Colorado in 2012. For data on state marijuana laws, I rely on ProCon.org, a nonprofit, nonpartisan public charity. ProCon.org provides a comprehensive analysis of the pros, cons, and supporting research on over 80 controversial issues, including state propositions and bills related to marijuana legislation. Previous studies (e.g., Chu and Townsend (2019), Cheng et al. (2023)) have utilized this data to examine the impacts of marijuana laws. I further verify the accuracy of the data by cross-referencing the dates with those listed on official state legislature websites. Table 1 shows the recreational marijuana legalization dates by states.

This study employs annual firm-level data from the Compustat database, with employment defined as the annual change in the number of employees (Ellul et al. (2018), Benmelech, Frydman, and Papanikolaou (2019)). Building on established literature (e.g., Ellul et al. (2018), Benmelech et al. (2019), Ersahin and Irani (2020), and Peng et al. (2025)), I incorporate controls for key firm-level variables such as leverage, Tobin's Q, cash flow, size, ROA, capital investment (CAPX/AT), and sales growth. These variables provide a comprehensive view of the firms' financial performance and investment capacity. Detailed descriptions of these variables are provided in the Appendix.

I also incorporate various macroeconomic indicators, including GSP growth from the U.S. Bureau of Economic Analysis (BEA), the unemployment rate from the U.S. Bureau of Labor Statistics, and the state-level EPU index from Baker, Davis, and Levy (2022b). Data on fiscal measures, specifically government spending scaled by GSP, are sourced from the State and Local Finance Initiative at the Urban Institute. Additionally, indicators of taxation freedom and labor market freedom are obtained from the Fraser Institute.[5] Taxation freedom measures the tax burden, with higher scores reflecting regions with lower, less restrictive taxes. Labor market freedom evaluates flexibility in hiring, firing, and regulatory constraints, awarding higher scores to regions with fewer restrictions. The analysis also includes data on RTW laws and public smoking bans, drawn from previous studies (e.g., Chava et al. (2020), Gao et al. (2020)) and state-level legislative websites.

I require firms to have complete data available for all variables used in the baseline regression, ensuring that each firm includes observations for all firm-level and dependent variables. The sample period spans from 2007 to 2023, covering 5 years before the first state legalized recreational marijuana in 2012.[6] Since this

[5]Both indices, scored from 0 to 10, provide insights into how local policies affect economic activity and business environment across states.

[6]This article notes that the sample period covers the COVID-19 pandemic, which severely distorted labor and capital markets. In the robustness checks, I consider such effects, and the results suggest an even stronger effect of RML on corporate employment when the COVID-19 period is excluded.

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TABLE 1

Recreational Marijuana Legalization Dates

Table 1 outlines when each state legalized recreational marijuana, based on data from *ProCon.org* and supplementary internet research. Maine legalized possession and home cultivation effective Jan. 30, 2017, but did not permit retail sales until 2020. Therefore, Maine's effective year is listed as 2017 to reflect the initial legal status for individuals, consistent with how similar milestones are recorded across jurisdictions.

State	Passed Year	Effective Year	Legalization Method
Alaska	2014	2015	Initiated ballot measure
Alabama			
Arizona	2020	2020	Initiated ballot measure
Arkansas			
California	2016	2016	Initiated ballot measure
Colorado	2012	2012	Initiated ballot measure
Connecticut	2021	2021	Legislative bill
Delaware	2023	2023	Legislative bill
Florida			
Georgia			
Hawaii			
Idaho			
Illinois	2019	2020	Legislative bill
Indiana			
Iowa			
Kansas			
Kentucky			
Louisiana			
Maine	2016	2017	Initiated ballot measure
Maryland	2022	2023	Legislatively referred ballot measure
Massachusetts	2016	2016	Initiated ballot measure
Michigan	2018	2018	Initiated ballot measure
Minnesota	2023	2023	Legislative bill
Mississippi			
Missouri	2022	2022	Initiated ballot measure
Montana	2020	2021	Initiated ballot measure
Nebraska			
Nevada	2016	2017	Initiated ballot measure
New Hampshire			
New Jersey	2020	2021	Legislatively referred ballot measure
New Mexico	2021	2021	Legislative bill
New York	2021	2021	Legislative bill
North Carolina			
North Dakota			
Ohio	2023	2023	Initiated ballot measure
Oklahoma			
Oregon	2014	2015	Initiated ballot measure
Pennsylvania			
Rhode Island	2022	2022	Legislative bill
South Carolina			
South Dakota			
Tennessee			
Texas			
Utah			
Vermont	2018	2018	Legislative bill
Virginia	2021	2021	Legislative bill
Washington	2012	2012	Initiated ballot measure
West Virginia			
Wisconsin			
Wyoming			

article needs to determine whether a firm is subject to RML based on its headquarters' state, the first step is to identify the firm's headquarters location. However, Compustat only provides information on the most recent headquarters state, which may introduce measurement errors since firms can change their headquarters. To address this, this article follows Bai, Fairhurst, and Serfling (2020) and Gao, Leung, and Qiu (2021) and uses SEC filings from EDGAR to identify each firm's headquarters. To prevent extreme values from skewing the analysis, all nonbinary firm-level and state-level variables are winsorized at the 1% level. Table 2 presents summary statistics for the primary variables, comprising nonmissing data across 57,070 observations.

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TABLE 2

Summary Statistics

Table 2 reports the summary statistics of the sample with nonmissing variables. All variables are measured at the annual frequency from 2007 to 2023.

	N	Mean	Std. Dev.	Min	Max
Employment	57,070	0.0661	0.3198	−0.6667	1.8919
Tobin's Q	57,070	3.2759	13.7802	−7.9438	175.2135
Cash flow	57,070	−0.4132	2.4335	−23.8333	0.4541
Leverage	57,070	1.0868	3.4797	0.0263	34.1142
ROA	57,070	−0.2779	1.8070	−17.5641	0.4570
CAPX/AT	57,070	0.0383	0.0571	0.0000	0.3623
Size	57,070	6.0706	2.6156	0.0060	11.8285
Sales growth	57,070	0.2000	0.5596	−0.8476	2.8857
GSP growth	57,070	3.9840	3.4461	−4.6000	13.7000
Unemployment	57,070	2.4596	1.7635	0.4333	8.0583
Government spending	57,070	13.3815	2.1867	9.4452	19.3650
EPU (log)	57,070	5.1603	0.4130	4.1654	6.4779
Taxation freedom	57,070	5.4967	1.2192	2.8755	8.4182
Labor market freedom	57,070	6.0138	1.0110	3.3601	8.4072
Blue state	57,070	0.4583	0.4983	0.0000	1.0000
Smoking ban	57,070	0.7616	0.4261	0.0000	1.0000
Right to work (RTW)	57,070	0.3736	0.4838	0.0000	1.0000
Delaware	57,070	0.5991	0.4901	0.0000	1.0000

On average, firms report an employment growth rate of 6.61%. Tobin's Q, a measure of firm valuation, has a mean of 3.2759, suggesting moderate market valuation relative to firm assets. In contrast, cash flow exhibits a negative mean of −0.4132, indicating that, on average, firms in the sample experience negative cash flow. The sales growth rate averages 20%, reflecting a moderate increase in sales. Firm leverage has a relatively high mean of 1.0868, signifying a substantial reliance on debt financing. The average firm size, as measured by the natural logarithm of total assets, is 6.0706. Additionally, the capital investment rate (CAPX/AT) averages 3.83%, while ROA is negative, with a mean of −0.2779.

From a state-level perspective, the average GSP growth rate is 3.98%, indicating moderate economic growth across regions. The average unemployment rate stands at 2.46%, while government spending accounts for 13.38% of GSP. The EPU index—log-transformed for scaling—has a mean value of 5.16, suggesting a moderate level of policy uncertainty during the sample period. On average, the taxation freedom score is around 5.50, and the labor market freedom scores about 6.01. Within the sample, 45.8% of states are classified as typically "blue," indicating general support for the Democratic Party. Approximately 37.4% have enacted RTW laws, 76.2% have implemented state-level smoking bans, and 59.9% are incorporated in Delaware.

IV. Empirical Results

A. Baseline Results

Table 3 presents the baseline results analyzing the impact of RML adoption on firm-level employment.[7] Across all model specifications, the findings consistently

[7]Note that the Delaware effect is absorbed in the model, and therefore, no estimated coefficient for it is reported in the table.

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TABLE 3

Baseline Results

Table 3 presents the effect of marijuana laws on firm-level employment. The employment is defined as the annual change in the number of employees. According to the literature, leverage, Tobin's Q, profitability, ROA, capital investment (CAPX/AT), cash flow, size, and sales growth are included as firm-level controls. GSP growth, unemployment rate, government spending, taxation freedom, labor market freedom, right-to-work law, blue state dummy, and smoking ban are included to control for the possible effect of aggregate conditions. The definitions are given in the Appendix. Firm- and year-fixed effects are included. Standard errors are clustered at the state level. Sample period is from 2007 to 2023. *, **, *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively. The Delaware effect is absorbed in the model, and therefore, no estimated coefficient for it is reported in the table.

	Dependent Variable: Employment			
	1	2	3	4
Panel A. Full Sample				
Post × Treat	−0.0278***	−0.0274***	−0.0300***	−0.0272***
	−0.0079	−0.0078	−0.0075	−0.0078
Treat	−0.0268	−0.0268	−0.017	−0.0176
	−0.0179	−0.0179	−0.0167	−0.0304
Cash flow		0.0148***	0.0028	0.0029
		−0.0021	−0.0036	−0.0036
Tobin's Q		0.0018***	0.0017	0.0017***
		−0.0004	−0.0004	−0.0004
Sales growth			0.1451***	0.1445***
			−0.0064	−0.0064
CAPX/AT			0.4225***	0.4198***
			−0.0618	−0.0618
ROA			0.0052	0.0051
			−0.0051	−0.0051
Size			0.0855***	0.0857***
			−0.0048	−0.0048
Leverage			0.0009	0.0009
			−0.0016	−0.0016
GSP growth				0.0022***
				−0.0008
Unemployment rate				0.0059*
				−0.0031
Government spending				−0.0016
				−0.0025
Tax freedom				−0.0004
				−0.0052
Labor market freedom				−0.0017
				−0.0048
Blue state				0.0139
				−0.0309
Log EPU				0.0068
				−0.0079
Smoking ban				0.0157
				−0.0136
Right to work				0.0271**
				−0.0136
Delaware				–
Firm FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Observations	57,070	57,070	57,070	57,070
Panel B. Small Firm Sample				
Post × Treat	−0.0485***	−0.0475***	−0.0452***	−0.0344**
	−0.0157	−0.0156	−0.0152	−0.016
Treat	−0.0275	−0.0288	−0.0171	−0.0275
	−0.0316	−0.0316	−0.0303	−0.064
Cash flow		0.0135***	0.0025	0.0024
		−0.0021	−0.0036	−0.0036

(continued on next page)

TABLE 3 (continued)
Baseline Results

	Dependent Variable: Employment			
	1	2	3	4
Tobin's Q		0.0017***	0.0017***	0.0017***
		−0.0004	−0.0004	−0.0004
Sales growth			0.1008***	0.1002***
			−0.0066	−0.0066
CAPX/AT			0.5474***	0.5439***
			−0.0826	−0.0824
ROA			0.0054	0.0054
			−0.0051	−0.0051
Size			0.0955***	0.0955***
			−0.0072	−0.0072
Leverage			0.0012	0.0012
			−0.0016	−0.0016
GSP growth				0.0026*
				−0.0016
Unemployment rate				0.008
				−0.0062
Government spending				−0.0068
				−0.0045
Tax freedom				−0.0008
				−0.0065
Labor market freedom				−0.0042
				−0.0063
Blue state				0.0239
				−0.0665
Log EPU				−0.0067
				−0.0168
Smoking ban				0.0294
				−0.0301
Right to work				0.0543*
				−0.0306
Delaware				–
Firm FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Observations	25,220	25,220	25,220	25,220

indicate a reduction in employment following RML adoption. In the initial specification (column 1), which includes only firm- and year-fixed effects, RML adoption is associated with a statistically significant negative effect on employment growth, corresponding to a 2.78 percentage point decline. In column 2, after controlling for Tobin's Q and cash flow, the negative association between RML adoption and employment persists, with the estimated coefficient remaining statistically significant at −0.0274.

Column 3 extends the analysis by incorporating additional firm-level control variables, where the interaction term (*Post* × *Treat*) continues to exhibit a significantly negative coefficient, suggesting an even stronger effect with a coefficient of −0.03. In column 4, after accounting for state-level macroeconomic and institutional variables, the results remain robust, indicating that the negative impact of RML adoption on employment is not attributable to broader economic conditions. From an economic perspective, the most conservative estimates indicate that employment growth decreases by −0.0272, corresponding to an approximate 2.72 percentage point reduction in employment growth.

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To account for geographic dispersion in workforce allocation, I follow the U.S. Small Business Administration's definition and restrict the sample to firms with 500 or fewer employees (Acs and Audretsch (1988), Autor, Cho, Crane, Goldar, Lutz, Montes, Peterman, Ratner, Villar, and Yildirmaz (2022)). This subsample, reported in Panel B of Table 3, reveals even stronger effects: Across all model variants, RML adoption leads to a reduction in employment growth of at least 3.44 percentage points. These results underscore the heightened sensitivity of smaller, geographically concentrated firms to labor market frictions induced by policy changes.

B. Dynamic Relationships

To address potential concerns regarding endogeneity and reverse causality, I adopt the approach of Bertrand and Mullainathan (2003) and Nunn and Qian (2011), placing particular emphasis on the timing of employment changes relative to the adoption of RML. This method can assess whether pretreatment trends in employment may bias the DiD estimates by violating the parallel trends assumption. If reverse causality were present, one would expect to observe a noticeable trend in employment prior to the adoption of marijuana legalization.

Figure 2 presents employment trends across the pre- and post-RML adoption periods. Graph A displays results for the full sample, while Graph B focuses on the subsample of small firms. Several important insights emerge. First, prior to RML adoption, there are no statistically significant differences in employment trends between the treated and control groups. This finding supports the validity of the parallel trends assumption, as both groups exhibit similar employment trajectories prior to legalization. Second, the estimated coefficients for RML are generally close to 0 and statistically insignificant in the pre-adoption period, but become significantly negative following legalization. This pattern lends support to the hypothesis that the observed decline in employment among treated firms is attributable to RML adoption, rather than to unobserved confounding factors. Third, the post-legalization period reveals a pronounced and statistically significant decline in employment within the treated group, with the effect becoming evident approximately 1 year after adoption and peaking between 3 and 4 years thereafter. This delayed response is intuitive, as firms may require time to adjust their organizational and employment structures in response to the evolving regulatory environment. The most substantial effects are observed in the fourth year following legalization, potentially reflecting the time required for marijuana markets to mature and for firms to adapt to the new economic and institutional landscape.

These findings reinforce the credibility of the identification strategy and underscore the robustness of the estimated negative employment effect. Taken together, Table 3 and Figure 2 provide compelling evidence that the adoption of RML exerts a meaningful and persistent dampening effect on firm-level employment growth.

C. Heterogeneity Analysis

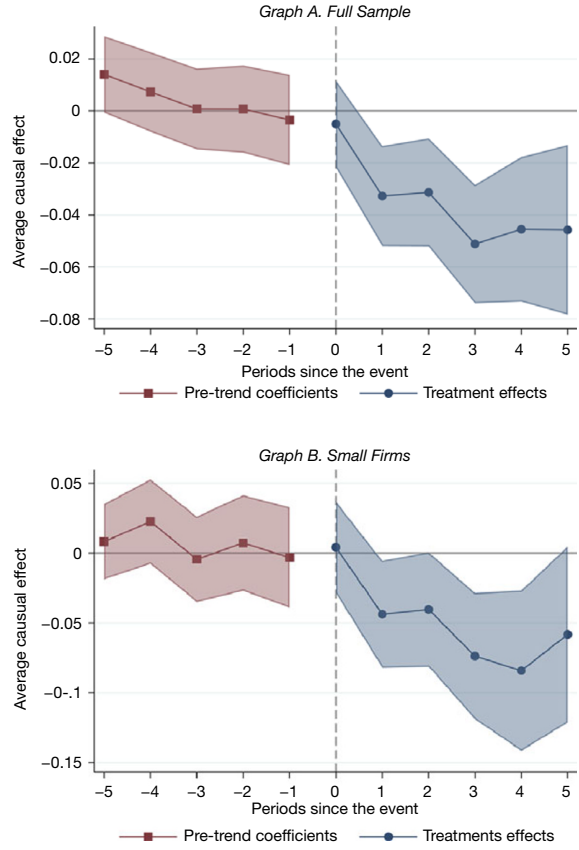
Having established a systematic decline in employment following RML adoption across the overall sample, I now extend the analysis by examining

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FIGURE 2

Timing of Employment Changes Around Marijuana Legalization

In Figure 2, I plot the coefficients of RMLs for corporate employment using equation (1). The horizontal axis represents the timeline, spanning from 5 years before the passage of RMLs to 5 years after their completion. The vertical axis illustrates the difference in employment between the treated and control groups. A 5% confidence interval is reported.



heterogeneity across firms. I hypothesize that the impact of marijuana legalization on employment is likely to vary according to the range of potential outcomes and the likelihood of policy-related shocks. Accordingly, I anticipate differences in the magnitude of employment changes across various firm- and region-level characteristics.[8]

1. High Skilled Versus Less Skilled Labor

The heterogeneity analysis begins by examining whether the employment effects of RML differ by the skill composition of a firm's workforce. Exploring whether RML has a greater effect on firms employing skilled or less-skilled labor is essential, given plausible arguments supporting both scenarios. Firms with skilled

[8]Robustness checks for the heterogeneity analysis and a discussion of the role of competition are provided in the Supplementary Material (Appendices B and C).

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labor may experience stronger negative effects, as these positions demand high levels of cognitive function, precision, and responsibility. Impairments resulting from increased marijuana use could lead to significant productivity losses and safety concerns, prompting firms to enforce strict drug policies and potentially reduce employment (Volkow et al. (2014)). Conversely, firms employing less-skilled labor might face a higher prevalence of marijuana use among employees, leading to increased absenteeism, reduced productivity, and higher turnover rates (Kaestner (1991)). These challenges could also result in employment reductions as firms strive to maintain operational efficiency.

Given the credible reasons for both types of firms to be adversely affected by RML, it is worthwhile to conduct an empirical investigation to determine which group experiences a more significant impact. Understanding the differential effects will provide valuable insights for policymakers and business leaders to develop targeted strategies that address the specific needs and vulnerabilities of each sector (DeSimone (2002)). This analysis not only contributes to the academic discourse on labor market dynamics but also informs practical approaches to mitigate the potential negative consequences of RML on employment across different segments of the workforce.

To assess these differential effects, we categorize firms based on the skill intensity of their respective industries, using the classification proposed by Belo et al. (2017). An industry is defined as high skilled if more than 50% of its workforce is employed in occupations requiring a specific vocational preparation (SVP) score of 7 or above. Firms operating within such industries are classified as high skilled; all others are designated low skilled. I incorporate this classification into the DiD framework by interacting a high-skill industry dummy with the RML treatment indicator. The key variable of interest is the triple interaction term *High-Skilled* × *Post* × *Treat*, which captures the differential impact of RML on employment across skill levels.

The results, presented in column 1 of Table 4, indicate that firms employing high-skilled labor experience a more substantial reduction in employment following the adoption of RML. Specifically, the coefficient is negative and statistically significant across both the full sample (−0.0476) and the small firm sample (−0.0912) at the 1% level, suggesting a robust and economically meaningful effect. In other words, employment in high-skilled firms declined by 4.76 and 9.12 percentage points more than in low-skilled firms, in the full sample and small firm sample, respectively, as a result of RML adoption.

2. The Role of Union Density

The subsequent analysis investigates whether the effect of RML on employment differs with respect to union density. The motivation for examining the interaction between recreational marijuana legalization and the strength of labor unions stems from an extensive body of literature illustrating unions' influence on corporate decision-making and firm outcomes. Prior research shows that labor unions, as powerful stakeholders, shape not only wages and employment contracts but also corporate policies, governance structures, and financial strategies. For example, Agrawal (2012) demonstrates that union shareholders can influence a firm's governance objectives, while Chyz, Leung, Li, and Rui (2013) report that

TABLE 4

Heterogeneity Analysis

Table 4 presents estimation results for the following regression:

$$Employment_{i,s,t} = \beta_1(Treat_{s,t} \times Post_{s,t} \times I_{i,s,t}) + \beta_2(Treat_s \times Post_t) + \beta_3(Treat_s \times I_{i,s,t}) + \beta_4(I_{i,s,t} \times Post_t) + \beta_5 I_{i,s,t} + \beta_6 Post_t + \beta_7 Treat_s + \rho_1 X_{i,s,t} + \rho_2 Z_{s,t} + \varphi_i + \vartheta_t + \varepsilon_{i,s,t},$$

where $I_{i,s,t}$ is a firm-specific, industry-specific, or state-specific characteristic. I categorize firms as high skilled or low skilled based on the Belo et al. (2017) index, with firms classified as high skilled if their index value exceeds 0.5. The High Union Density dummy variable equals one for firms operating in industries with above-median union density. Based on definitions from a Pew Research Center survey, I classify Oklahoma, California, Michigan, Florida, and Colorado as high-dispensary states; firms located in these states are coded as 1, and 0 otherwise (High Dispensary Density). Firms with respect index values above the sample median are classified as high-respect culture firms. Detailed definitions of these variables are provided in the Appendix. Firm- and year-fixed effects are included. Standard errors are clustered at the state level. The sample period is from 2007 to 2023. *, **, *** indicate statistical significance at the 10%, 5%, and 1% level, respectively.

	Dependent Variable: Employment			
	High Skilled	High Union Density	High Dispensary Density	High Respect
	1	2	3	4
Panel A. Full Sample				
Post × *Treat* × *Indicator*	−0.0476***	−0.0209*	−0.0434***	−0.0243**
	(0.0123)	(0.0119)	(0.0134)	(0.0101)
Other controls	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Observations	53,016	57,070	57,070	32,654
Panel B. Small Firm Sample				
Post × *Treat* × *Indicator*	−0.0912***	−0.0384*	−0.0618**	−0.0238
	(0.0225)	(0.0233)	(0.0257)	(0.0194)
Other controls	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Observations	23,652	25,220	25,220	8,488

unions affect corporate tax aggressiveness, indicating the substantial pressure unions can exert on management decisions. Similarly, Agrawal and Matsa (2013) find that labor unemployment risk affects corporate financing choices, and Klasa, Maxwell, and Ortiz-Molina (2009) provide evidence of unions influencing the strategic use of corporate cash holdings during collective bargaining. Tong and Huang (2018) show that unions can affect corporate cash policies internationally, and Huang, Jiang, Lie, and Que (2017) highlight that unions also shape executive compensation structures. Collectively, these findings indicate that labor unions are active participants in shaping firm-level dynamics, potentially altering how firms respond to regulatory shocks, including RML.

Building on the baseline finding that RML reduces firm-level employment, I explore whether this effect varies across environments with different levels of unionization. On one hand, unions may mitigate negative employment outcomes by securing stronger worker protections, collectively bargained contracts, and wage floors—mechanisms that can buffer against layoffs or wage cuts. On the other hand, high union density may exacerbate employment losses by increasing labor rigidity and cost pressures, especially when firms face regulatory uncertainty and shifting consumer preferences. Union contracts often include elevated wage standards and stricter hiring and firing rules, which, while protective in stable conditions, can limit firms' agility in adjusting to adverse shocks.

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Moreover, as unions influence governance and financial decisions (Klasa et al. (2009), Tong and Huang (2018)), they may reduce managerial flexibility, constrain liquidity, and entrench fixed costs—factors that could amplify employment declines in the wake of RML. Understanding this interaction helps clarify whether unions act as a stabilizing force or a frictional constraint in the context of regulatory change.

To measure the influence of unionization, I match a firm's union density to the state-level rate of unionization provided by the UNIONSTATS database (Hirsch and Macpherson (2003)), which offers estimates of union membership and coverage by state. I create a binary variable equal to 1 if a firm operates in a state with above-median union coverage rates, and 0 otherwise. I then include an interaction term between the RML measure (*Post* × *Treat*) and this union density dummy in the baseline regression model. The primary interest lies in the coefficient of this interaction term. As shown in column 2 of Table 4, the interaction coefficient is significantly negative, indicating that the adverse effect of RML on employment is more pronounced for firms in industries with higher union density. In other words, stronger unionization appears to amplify the negative impact of RML on employment. In terms of magnitude, employment growth declined by at least 2.09 percentage points more for firms in highly unionized industries compared to those in less unionized sectors.

3. Dispensary Density as Treatment Intensity

To further investigate the heterogeneous effects of RML on firm-level employment, I exploit variation in exposure intensity by leveraging the geographic distribution of marijuana dispensaries. Although RML represents a uniform policy shift across treated states, actual exposure to the legalized marijuana market varies markedly across regions due to differences in local regulatory environments and market saturation. In this context, dispensary density serves as a meaningful proxy for the intensity of treatment.

This approach is consistent with prior literature emphasizing that spatial access to marijuana markets plays a critical role in shaping post-legalization outcomes. For instance, Mair, Freisthler, Ponicki, and Gaidus (2015) show that higher dispensary density is positively associated with increased rates of marijuana abuse and dependence at the neighborhood level, underscoring the behavioral consequences of localized access. Similarly, Smith (2020) finds that the availability of medical marijuana dispensaries is linked to significant reductions in opioid-related hospitalizations and deaths. This effect is attributed, at least in part, to the substitution of marijuana for opioids in pain management, highlighting the importance of dispensary accessibility in influencing patterns of substance use. Together, these studies underscore the empirical relevance of dispensary density as a proxy for policy exposure. Based on this literature, I hypothesize that the effects of RML on employment will be more pronounced for firms located in areas with higher dispensary density.

To test this hypothesis, I construct a dummy variable equal to one for firms operating in states characterized by high dispensary density. Based on definitions

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from a Pew Research Center survey,[9] we classify Oklahoma, California, Michigan, Florida, and Colorado as high-dispensary states; firms located in these states are coded as 1, and 0 otherwise. I then interact this high-dispensary dummy with the main policy variable (*Post* × *Treat*). The difference-in-difference-in-differences (DDD) specification includes this interaction term, the high-dispensary dummy, and relevant control variables. As reported in column 3 of Table 4, the coefficient on the interaction term is negative and statistically significant at the 5% level. This finding supports the hypothesis that firms located in areas with greater dispensary density experience more pronounced negative employment effects following recreational marijuana legalization.

4. The Role of Corporate Culture

Last, corporate culture plays a pivotal role in shaping organizational policies, employee behavior, and overall firm performance. Firms with strong corporate cultures that emphasize strict adherence to policies, high ethical standards, and employee well-being may respond differently to recreational marijuana legalization compared to those with more relaxed or laissez-faire cultures. According to Schein (2010), corporate culture influences how organizations adapt to external changes and internal challenges. In the context of RML, firms with more permissive cultures might adopt a more lenient approach, potentially affecting productivity and safety. However, in firms with strict, zero-tolerance policies toward drug use, employees may be deterred from using marijuana due to fear of disciplinary action. In such environments, the negative effects of drug use on productivity may be minimized, potentially reducing any adverse impact on employment.

To examine the role of corporate culture in shaping the relationship between RML and employment, we use the culture indices developed by Li, Mai, Shen, and Yan (2021). They develop a corporate culture dictionary using machine learning, applying it to earnings call transcripts to quantify firm-level corporate culture across five dimensions: innovation, integrity, quality, respect, and teamwork.[10] Their findings show that higher values on the respect index are positively associated with a firm's likelihood of being recognized as one of the best employers, while a stronger innovative culture correlates with higher R&D spending and more patents.

In this analysis, I specifically focus on firms with a high respect culture, as this dimension is closely associated with diversity and tolerance. I define high-respect culture firms as those with respect index values above the sample median. Consistent with the previous analyses, I incorporate an interaction term between the high-respect culture dummy and the RML variable. The results, presented in column 4 of Table 4, indicate that firms with a more tolerant culture experience a greater reduction in employment growth following the adoption of RML.[11] This result aligns with our broader findings and highlights the critical role of corporate culture

[9]https://www.pewresearch.org/short-reads/2024/02/29/most-americans-now-live-in-a-legal-marijuana-state-and-most-have-at-least-one-dispensary-in-their-county/.

[10]A higher index value indicates that firms have stronger culture. For example, an increase in a firm's teamwork index indicates that this firm emphasises teamwork in its culture.

[11]Note that the number of observations decreases noticeably because the corporate culture indices are not available for all listed firms, resulting in some missing data.

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in moderating the effects of policy shocks. Specifically, permissive cultures may inadvertently facilitate conditions that magnify the adverse labor market impacts of legalization.

D. RML and Other Corporate Outcomes

Thus far, the findings suggest that the legalization of recreational marijuana leads to a reduction in firm-level employment. Building on these results, this article further explores whether RML also affects other dimensions of corporate behavior and performance. Specifically, I extend our analysis to examine capital investment (CAPX/AT), R&D expenditure (R&D/AT), sales growth, firm value (measured by Tobin's Q), and labor productivity, estimating separate firm-level DiD regressions for each outcome.[12] These regressions incorporate the same set of control variables, firm-fixed effects, and year-fixed effects used in the employment analysis.[13]

The results, presented in Table 5, suggest that RML has a broader impact on corporate outcomes beyond employment. In the full sample, capital investment declines by 0.22 percentage points and R&D intensity by 3.24 percentage points (both relative to total assets) and both effects are statistically significant (see columns 1 and 2). As shown in column 3, sales growth also falls significantly, by 4.27 percentage points, indicating a more general deceleration in firm activity. These adverse effects tend to be more pronounced among smaller firms, with R&D intensity falling by 5.91 percentage points. Labor productivity declines by approximately

TABLE 5

Recreational Marijuana Legalization and Other Corporate Outcomes

Table 5 presents supplementary analyses examining the impact of marijuana legalization on various corporate outcomes. The dependent variable for each model is specified in the first row. All other model specifications remain consistent with the baseline analysis. Standard errors are clustered at the state level. The sample period spans from 2007 to 2023. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

	CAPX/AT	R&D/AT	Sales Growth	Tobin's Q	Labor Productivity
	1	2	3	4	5
Panel A. Full Sample					
Post × *Treat*	−0.0022*	−0.0324***	−0.0427***	−0.1803	−29.1478*
	(0.0014)	(0.0091)	(0.0137)	(0.2587)	(16.6837)
Other controls	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Observations	57,070	57,070	57,070	57,070	57,070
Panel B. Small Firm Sample					
Post × *Treat*	−0.0047*	−0.0591***	−0.0539*	−0.3572	−29.2455
	(0.0028)	(0.0210)	(0.0294)	(0.5988)	(32.6053)
Other controls	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Observations	25,220	25,220	25,220	25,220	25,220

[12]Following Mitton (2012), I measure firm-level labor productivity as the ratio of total sales to the number of employees.

[13]Note that I do not include a variable as a control when it serves as the dependent variable. For example, although capital investment is used as a control in the baseline specification, we do not control for it when it is the dependent variable.

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29 units (measured as sales per employee), which is marginally significant at the 10% level. The effect on Tobin's Q is negative but not statistically significant. Collectively, this analysis suggests that RML has detrimental effects not only on employment but also on broader aspects of corporate investment and performance.

E. RML and Corporate Innovation

Building on the earlier finding that RML disproportionately affects high-skilled firms—where innovation-related activities are typically more concentrated—this article further investigates how marijuana legalization influences firm-level innovation outcomes. This extension is motivated by a growing literature suggesting that regulatory or institutional shifts, such as public health policies or labor-related reforms, can have meaningful implications for corporate innovation (e.g., Gao, Hsu, and Li (2018), Gao et al. (2020)). High-skilled firms are more likely to engage in knowledge-intensive production and long-term innovation strategies, making them particularly sensitive to policy environments that affect employee performance, focus, and firm culture.

To move beyond input-based measures such as R&D expenditures, this analysis examines a range of patent-based indicators that capture the quantity, quality, and type of innovation. Patent data are drawn from Arora, Belenzon, and Sheer (2021)), who develop a comprehensive and dynamically updated matching algorithm to link U.S. patents with Compustat firms, accounting for name changes, mergers, and changes in corporate ownership structures. This allows for accurate firm-level attribution of innovation output. The quality- and strategy-based measures of innovation follow Kogan, Papanikolaou, Seru, and Stoffman (2017), including forward citations as an indicator of technological impact, and classifications into exploratory versus exploitative patents to reflect innovation orientation.[14] Following the innovation literature (e.g., He and Tian (2013)), patent and citation counts are set to 0 for firms without available patent or citation information from these two databases.

The results, presented in Panel A of Table 6, indicate a consistent negative effect of RML on innovation outcomes. Following RML adoption, the number of granted patents fell by 4.05% and patent applications by 6.02%, both statistically significant. In addition, columns 3 and 4 indicate that measures of patent quality deteriorate: Total forward citations decline by 24.6%, and average citations per patent decrease by 15.5%, suggesting that not only is less innovation produced, but its technological relevance is also diminished. Invention productivity also declines significantly[15].

I further distinguish between exploratory and exploitative patents to assess the nature of firms' innovation strategies. Both categories exhibit significant declines—2.72% and 8.11%, respectively—implying a broad-based contraction in innovation activity (see columns 5 and 6). The sharper drop in exploitative

[14]This study classifies patents as exploratory if their combination of Cooperative Patent Classification (CPC) codes represents a novel configuration not previously observed in the data set. Conversely, patents are classified as exploitative if they reuse existing CPC code combinations, indicating an extension or refinement of established technological paths.

[15]I follow Fich, Nguyen, and Petmezas (2023) to use the ratio of number of granted patents to number of employees to measure invention productivity.

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TABLE 6

Recreational Marijuana Legalization and Innovation Outcomes

Table 6 presents supplementary analyses examining the impact of marijuana legalization on innovation outcomes. The dependent variable for each model is specified in the first row. All other model specifications remain consistent with the baseline analysis. Standard errors are clustered at the state level. The sample period spans from 2007 to 2023. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

	Granted Patents	Patent Applications	Total Citations	Average Citations	Exploratory Patents	Exploitative Patents	Patents per Employee
	1	2	3	4	5	6	7
Panel A. Full Sample							
Post × *Treat*	−0.0405**	−0.0602***	−0.2462***	−0.1548***	−0.0272*	−0.0811***	−2.3607***
	(0.0192)	(0.0194)	(0.0341)	(0.0192)	(0.0157)	(0.0134)	(0.7110)
Other controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	57,070	57,070	57,070	57,070	57,070	57,070	57,070
Panel B. Small Firm Sample							
Post × *Treat*	−0.0349*	−0.0492**	−0.1606***	−0.0942***	−0.0073	−0.0765***	−3.5413**
	(0.0208)	(0.0231)	(0.0392)	(0.0245)	(0.0148)	(0.0167)	(1.5991)
Other controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	25,220	25,220	25,220	25,220	25,220	25,220	25,220

patents may indicate that firms are pulling back from near-term, incremental innovation, while the reduction in exploratory patents suggests hesitancy to pursue long-horizon, riskier projects. Last, a decline in patents per employee is observed, further reinforcing the view that RML weakens firms' innovation efficiency (see column 7).

Collectively, these findings provide robust evidence that marijuana legalization adversely affects not only innovation input but also output, quality, and strategic direction—particularly in sectors reliant on high-skilled, cognitively intensive labor—highlighting broader implications for long-term firm value and growth potential.

F. Robustness Checks

To ensure the reliability and robustness of the findings, this article conducts a series of additional checks. These address various aspects of the analysis, including placebo tests, the influence of economic crises, potential confounding factors, and the use of alternative winsorization techniques. Collectively, these robustness checks reinforce the validity of the main results and provide further evidence that the legalization of recreational marijuana has a significantly negative effect on employment.

1. Impact of the COVID-19 Pandemic and the Global Financial Crisis

Given the potential influence of both the COVID-19 pandemic and the 2007–2009 Global Financial Crisis on firm behavior—particularly in relation to liquidity and investment—this analysis conducts additional tests to mitigate any distortions arising from these periods. These checks are essential, as one might argue that the observed reduction in employment could be attributed to these macroeconomic shocks rather than to the enactment of RMLs, despite the inclusion of controls for aggregate macroeconomic conditions. By restricting the sample to the years

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TABLE 7

Robustness Checks

Table 7 presents additional tests that support the main findings. Employment is defined as the annual change in the number of employees. Column 1 restricts the sample period to 2010–2019, thereby excluding the periods of the COVID-19 pandemic and the financial crisis. Columns 2 and 3 show results after excluding financial and utility firms as well as firms operating in cannabis-related industries, respectively. Column 4 re-performs the DiD analysis using the implementation year of RMLs. Column 5 reports the results after further controlling for other law adoptions. Column 6 replicates the baseline specification with all variables winsorized at the 5% level. All other model specifications remain consistent with the baseline analysis. Columns 7 and 8 use sales tax per capita and cannabis sales per capita, respectively, as proxies for cannabis intensity (not based on the DiD post × treat interaction). Standard errors are clustered at the state level. The sample spans 2007 to 2023. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

	Dependent Variable: Employment							
	Exclude COVID-19 and Crisis Periods	Exclude Financial & Utility Firms	Excluding Cannabis-Related Industries	Implementation Year	Controlling Other Laws	Winsorized 5%	Sales Tax per Capita	Sales per Capita
	1	2	3	4	5	6	7	8
Panel A. Full Sample								
Post × *Treat*	−0.0305***	−0.0329***	−0.0237***	−0.0271***	−0.0287***	−0.0129***		
	(0.0098)	(0.0090)	(0.0077)	(0.0079)	(0.0079)	(0.0040)		
CSA law					−0.0526**			
					(0.0223)			
PP law					−0.0004			
					(0.0203)			
UD law					0.0355			
					(0.0255)			
BC law					−0.0142			
					(0.0248)			
FP law					0.0135			
					(0.0216)			
Sales tax per capita							−0.0063**	
							(0.0029)	
Sales per capita								−0.0047**
								(0.0020)
Other controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	45,871	46,136	54,799	57,070	57,070	57,070	29,353	29,966

(continued on next page)

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TABLE 7 (continued)

Robustness Checks

	Dependent Variable: Employment							
	Exclude COVID-19 and Crisis Periods	Exclude Financial & Utility Firms	Excluding Cannabis-Related Industries	Implementation Year	Controlling Other Laws	Winsorized 5%	Sales Tax per Capita	Sales per Capita
	1	2	3	4	5	6	7	8
Panel B. Small Firm Sample								
Post × *Treat*	−0.0398**	−0.0426**	−0.0283*	−0.0340**	−0.0388**	−0.0159**		
	(0.0178)	(0.0192)	(0.0163)	(0.0161)	(0.0161)	(0.0079)		
CSA law					−0.0831**			
					(0.0380)			
PP law					0.0112			
					(0.0374)			
UD law					0.0618			
					(0.0471)			
BC law					−0.0255			
					(0.0419)			
FP law					−0.0171			
					(0.0433)			
Sales tax per capita							−0.0114*	
							(0.0059)	
Sales per capita								−0.0083**
								(0.0044)
Other controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	20,938	19,502	23,458	25,220	25,220	25,220	12,149	12,572

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2010–2019, thereby excluding the impact of both crises, this article confirms that the negative association between RMLs and firm-level employment is not driven by these external events. The results, shown in column 1 of Table 7, continue to demonstrate a statistically significant negative relationship between RMLs and employment, even prior to 2020.

2. Exclusion of Financial and Utility Firms

This article also conducts robustness checks to address potential concerns related to sample selection. In corporate finance research, it is standard practice to exclude firms in the financial sector (SIC codes 6000–6999) and the utility sector (SIC codes 4900–4999) due to their unique regulatory requirements, particularly concerning liquidity. Although these firms are not excluded in the primary analysis, I perform additional regressions excluding financial and utility firms to ensure that their inclusion does not bias the results. The results, presented in column 2 of Table 7, show that the coefficient on the interaction term *Post* × *Treat* remains significantly negative, consistent with the main findings in Table 3. This suggests that the inclusion of financial and utility firms does not drive the primary results.

3. Excluding Cannabis-Related Industries

To ensure that the findings are not driven by firms directly involved in the cannabis industry, I reestimate the baseline model after excluding firms operating in sectors most likely to be affected by RML through direct market participation. Specifically, I exclude firms with SIC codes 2833, 2834, 2084, and 5122 (codes commonly associated with medicinal botanicals, pharmaceutical preparations, and retail or wholesale distribution of cannabis-related products). The results, reported in column 3 of Table 7, remain economically and statistically similar to the baseline, suggesting that the estimated effects are not driven solely by cannabis-sector firms.

4. Implementation Year of RML

In the baseline analysis, the adoption year of RMLs is used to perform the DiD analysis. However, one could argue that using the year when state marijuana legislation is implemented may more accurately capture actual changes in productivity and investment behavior, as opposed to speculative or anticipatory responses, thereby raising questions about the baseline approach. To address this concern, this analysis instead uses the implementation year of RMLs to conduct a staggered DiD analysis, with the results presented in column 4 of Table 7. The findings clearly indicate that RML reduces firm-level employment.

5. Further Controlling for Other Law Adoption

Given that previous studies have demonstrated that legal and regulatory changes can significantly influence corporate decisions in the context of concurrent legal shifts (e.g., Bertrand and Mullainathan (2003), Fich, Harford, and Yore (2016), Karpoff and Wittry (2018), and Foroughi, Marcus, Nguyen, and Tehranian (2022)), this article further controls for various state-level legal frameworks. These include BC laws, Poison Pill (PP) legislation, CSA laws, Fair Price (FP) laws, and Universal Demand (UD) laws. This approach ensures that the findings are not confounded by other regulatory changes. The results reported in column 5 of Table 7 indicate that marijuana legislation continues to have a negative impact on firm-level employment.

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6. Winsorization at 5%

I next address concerns regarding the treatment of outliers. While the baseline analysis applies winsorization at the top and bottom 1%, this choice may be seen as somewhat arbitrary. For robustness, I also winsorize all nonbinary variables at the top and bottom 5%, which is another common practice in corporate finance research (Mitton (2022)). I then rerun the DiD regression using variables winsorized at 5%, and the results, reported in column 6 of Table 7, show that legalization of recreational marijuana continues to reduce employment.

7. Alternative Proxies for RML Intensity

Finally, to address concerns regarding the timing of the employment effects—particularly the observation that the strongest impact occurs several years after the passage of RML—this article conducts additional robustness checks using more granular measures of marijuana market activity. While the baseline specification captures the legal status of recreational marijuana through a binary *Post* × *Treat* indicator, it does not account for variation in how quickly and extensively the market develops across states. To better reflect this heterogeneity, I incorporate continuous measures of market intensity: marijuana sales per capita and marijuana sales tax revenue per capita. These indicators allow the analysis to capture both the scale and pace of market expansion, reflecting actual consumer participation and the maturity of the legal marijuana industry in each state year. This approach helps explain why the employment effects may intensify over time in some states but remain muted in others.

The results, presented in columns 7 and 8 of Table 7, indicate that both sales tax per capita and marijuana sales per capita are negatively and significantly associated with firm-level employment. Specifically, in the full sample analysis, a 1% increase in marijuana sales per capita is associated with a 0.0047 percentage point reduction in employment, while a 1% increase in sales tax per capita corresponds to a 0.0063 percentage point decline. Similar findings are observed in the small-firm sample reported in Panel B of Table 7. These results reinforce the interpretation that the delayed employment effects observed in the baseline model are likely driven by differences in the intensity of marijuana market development. In states where the marijuana market expanded more rapidly and reached higher levels of consumer activity, firms appear to experience more pronounced adverse effects on employment.

8. Placebo Tests

To test whether the estimated treatment effect could be driven by spurious correlations or broader time trends unrelated to the actual policy, this article conducts a placebo analysis by randomly assigning treatment years to RML-adopting states and reestimating the ATT 500 times. The distribution of these placebo ATT estimates has a mean of −0.0014 and a standard deviation of 0.0091, as shown in Figure 3. The minimum value across the placebo simulations is −0.0269, which is still less negative than the true ATT of −0.0272. As such, none of the 500 placebo estimates are as extreme as the actual effect, yielding a one-sided p-value of <0.002. This provides compelling evidence that the estimated employment impact of RMLs is unlikely to be driven by chance or unrelated time effects.

In addition to the permutation-based placebo test using randomized treatment timing, this analysis further assesses the specificity of the findings by conducting

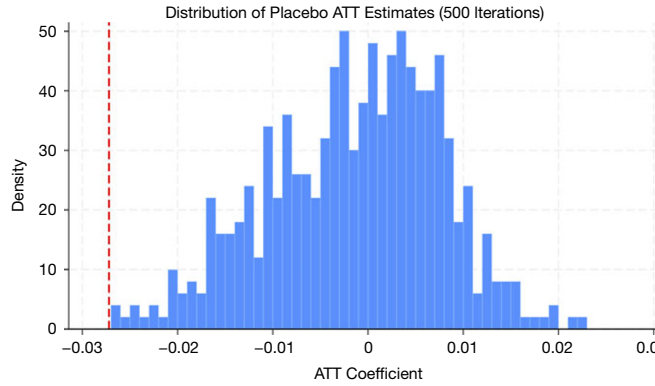
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FIGURE 3

Placebo Distribution of ATT Estimates across 500 Simulated Treatment Years

Figure 3 shows the distribution of placebo ATT estimates from 500 simulated iterations. In each iteration, the treatment year for the treated states was randomly reassigned, and the ATT was reestimated using the same difference-in-differences (DiD) specification. The red dashed line marks the actual estimated ATT of −0.0272, representing the effect of RMLs on employment growth using the correct policy implementation years.



policy-based placebo tests using CSA laws and BC laws. While these corporate governance policies may plausibly influence certain firm-level decisions, they are not expected to affect employment outcomes through the same heterogeneity channels as RML, such as marijuana dispensary prevalence, unionization rates, or skill intensity. Because both CSA and BC laws were enacted prior to the observation period in all treated states, there is no natural pre-post policy variation. To construct a valid placebo comparison, I assign a pseudo-treatment year (e.g., 2014) and define a constructed post-treatment indicator accordingly. I then interact

TABLE 8

Placebo Test Using Control Share Acquisition (CSA) Laws

Table 8 reports results from a placebo test based on the historical adoption of Control Share Acquisition (CSA) laws. A pseudo-treatment year (2014) is assigned, and a constructed post-treatment indicator is interacted with CSA policy status. The estimation follows the same triple-differences (DDD) specification used in the heterogeneity analysis (Table 4). Panel A presents results for the full sample, while Panel B focuses on the small-firm subsample (fewer than 500 employees). Across all specifications, the placebo interaction coefficients are small and statistically insignificant. Standard errors are clustered at the state level.

	Dependent Variable: Employment			
	High Skilled	High Union Density	High Dispensary Density	High Respect
	1	2	3	4
Panel A. Full Sample				
Post × *Treat* × *Indicator*	−0.0199	0.0049	−0.0196	−0.0113
	(0.0123)	(0.0125)	(0.0157)	(0.0091)
Other controls	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Observations	51,636	57,025	56,567	32,080
Panel B. Small Firm Sample				
Post × *Treat* × *Indicator*	−0.0190	0.0051	−0.0064	−0.0310
	(0.0254)	(0.0275)	(0.0307)	(0.0287)
Other controls	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Observations	22,624	25,186	24,879	8,250

TABLE 9

Placebo Test Using Business Combination (BC) Laws

Table 9: presents results from a placebo test based on the historical adoption of Business Combination (BC) laws. A pseudo-treatment year (2014) is used to construct a post-treatment indicator, which is then interacted with BC law status. The analysis replicates the triple-differences (DDD) framework applied in Table 4. Panel A reports results for the full sample, and Panel B presents results for the small-firm subsample (fewer than 500 employees). Placebo interaction terms are uniformly small and statistically insignificant across specifications. Standard errors are clustered at the state level.

	Dependent Variable: Employment			
	High Skilled	High Union Density	High Dispensary Density	High Respect
	1	2	3	4
Panel A. Full Sample				
Post × Treat × Indicator	−0.0123	0.0009	−0.0248	−0.0033
	(0.0083)	(0.0085)	(0.0217)	(0.0076)
Other controls	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Observations	50,521	55,939	56,691	31,605
Panel B. Small Firm Sample				
Post × Treat × Indicator	−0.0049	0.0072	0.0030	0.0047
	(0.0169)	(0.0164)	(0.0374)	(0.0222)
Other controls	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Observations	21,767	24,423	24,947	8,013

this indicator with the historical policy adoption status and estimate the same DDD specification used in the heterogeneity analysis presented in Table 4.

The results, reported in Tables 8 and 9 (where the former presents findings for CSA laws and the latter for BC laws), show that the estimated placebo effects are uniformly small and statistically insignificant. Across both the full sample and the small-firm subsample, none of the coefficients on placebo policy interactions—across heterogeneity measures such as dispensary density, unionization, skill intensity, or related values—yield meaningful results. These findings provide additional evidence that the main results are not driven by spurious correlations or artifacts of model specification. The observed employment effects appear specific to the adoption of RMLs.

V. Conclusion

This study provides robust evidence that the legalization of recreational marijuana has a significant negative impact on firm-level employment. These adverse effects are most pronounced among firms with a high reliance on skilled labor, those located in states with greater dispensary density, firms operating in highly unionized industries, and those characterized by permissive or autonomy-oriented corporate cultures. Beyond employment, RML reduces capital investment, R&D intensity, and sales growth, while marginally depressing labor productivity. Innovation outcomes also deteriorate following legalization; patent volume, quality (as measured by citations), and strategic breadth (exploratory versus exploitative patents) all decline. This suggests that RML introduces a new, under-recognized source of organizational friction with meaningful implications for long-term firm value

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creation. I further validate main findings using a battery of robustness tests and placebo designs, including the exclusion of crisis years, sectoral filters, alternative policy timing, intensity-based measures, and policy-based falsification exercises. The consistency of the evidence across specifications and samples reinforces the credibility and policy relevance of the conclusions.

These findings have important implications for both policymakers and financial economists. Regulators should recognize the heterogeneity in how RML affects firms and consider complementary policies, such as enhanced workplace safety standards or targeted support for high-risk industries, to mitigate adverse effects. Although the policy is sector specific, the results suggest that its impact spills over into the broader corporate landscape, affecting firms well outside the marijuana industry. For firms, especially those operating in cognitively demanding sectors or permissive cultural environments, the results indicate a need for adaptive HR strategies and more rigorous internal controls post-legalization.

Future research may explore the long-run performance consequences of RML, the role of regulatory design (e.g., opt-out provisions or THC content limits), and how labor market institutions mediate firm-level responses. Moreover, assessing the cross-border or spillover effects of legalization on neighboring jurisdictions could offer further insight into the economic and competitive dynamics unleashed by marijuana reform. This study offers an empirical foundation for such inquiries, bridging gaps between public health policy and corporate finance.

Appendix. Variable Definitions and Sources

Variable	Definition	Sources
Employment	Annual change in the number of employees	Compustat
Leverage	The ratio of total debt to total assets	Compustat
Size	The log of total assets plus one	Compustat
Tobin's Q	(The market value of equity plus book value of assets minus book value of equity) divided by the book value of the asset.	Compustat
Cash flow	(Income before extraordinary items plus depreciation and amortization) divided by the book value of total assets.	Compustat
Sales growth	The percentage growth rate of sales	Compustat
ROA	Dividing net income by total assets	Compustat
CAPX/AT	The ratio of capital expenditure to total assets.	Compustat
R&D/AT	The ratio of R&D expenditure to total assets.	Compustat
GSP growth	One year percentage change in per capita state-level gross domestic product	U.S. Bureau of Economic Analysis
Unemployment	The state-level unemployment rate is the percentage of the labor force that is unemployed but actively seeking employment	U.S. Bureau of Labor Statistics
Government spending	Government spending refers to the total amount of money that a government expends on goods, services, and public programs. It is calculated as the ratio of government spending to GDP	State and Local Finance Initiative
EPU	Economic policy uncertainty (EPU) is defined as a measure of uncertainty regarding economic policies, specifically at the state level. The EPU index is constructed by analyzing the frequency of news articles referencing terms related to the economy, policy, and uncertainty within specific states	Baker et al. (2022a)
Taxation freedom	The *Taxation Freedom (Subnational)* score by the Fraser Institute measures the level of taxation freedom in U.S. states, ranking from 0 (high tax burden) to 10 (low tax burden). Higher scores indicate states with lower tax burdens on individuals and businesses, promoting greater economic freedom	The Fraser Institute

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Variable	Definition	Sources
Labor market freedom	The Labor Market Freedom score by the Fraser Institute assesses the degree of economic freedom in U.S. states regarding labor market policies. It is ranked from 0 to 10, with higher scores indicating fewer regulatory constraints, lower minimum wages, less restrictive union laws, and more flexibility in hiring and employment practices. States with higher labor market freedom scores are generally seen as more favorable for business and employment growth	The Fraser Institute
Right to Work	An indicator variable that takes the value of 0 prior to the enactment of Right-to-work (RTW) laws, and 1 in all subsequent years, including the year of enactment	Chava et al. (2020), Internet Search
Smoking ban	An indicator variable that equals 1 if a firm is headquartered in a state that has enacted a workplace smoking ban, beginning in the year of enactment and continuing in all subsequent years, and 0 otherwise	Gao et al. (2020), Olson et al. (2023), Internet Search
Blue state	An indicator variable equal to 1 if a firm is headquartered in a state that usually supports the Democratic Party, and 0 otherwise	Federal Election Commission; The author
Delaware	An indicator variable that takes the value of 1 if a firm incorporated in Delaware, and 0 otherwise	Compustat
High union density	The high union density dummy variable equals 1 for firms operating in industries with above-median union density	The author calculation
High skilled	We categorize firms as high skilled or low skilled based on the Belo et al. (2017) index, with firms classified as high skilled if their index value exceeds 0.5. That is, more than 50% of the jobs in those industries required the highest skills	The author calculation
High-dispensary density	Firms operating in states characterized by high dispensary density. Based on definitions from the Pew Research Center, we classify Oklahoma, California, Michigan, Florida, and Colorado as high-dispensary states; firms located in these states are coded as 1, and 0 otherwise	Pew Research Center, https://www.pewresearch.org/short-reads/2024/02/29/most-americans-now-live-in-a-legal-marijuana-state-and-most-have-at-least-one-dispensary-in-their-county/
High respect	Firms with respect index values above the sample median are classified as high-respect culture firms	The author calculation
Labor Productivity	Following Mitton (2012), we measure firm-level labor productivity as the ratio of total sales to the number of employees	The author calculation
Granted patents	Number of granted patents for each firm at fiscal year *t*	Arora et al. (2021)
Patent applications	Number of patent applications for each firm in fiscal year *t*	Arora et al. (2021)
Total citations	The number of total forward citations for each firm in fiscal year *t*	Kogan et al. (2017)
Average citations	The ratio of total citations to granted patents for each firm at fiscal year *t*	Kogan et al. (2017), the author calculation
Exploratory patents	We classify patents as exploratory if their combination of Cooperative Patent Classification (CPC) codes represents a novel configuration not previously observed in our data set. Conversely, patents are deemed exploitative if they reuse existing CPC code combinations, indicating an extension or refinement of established technological paths	The author calculation, Kogan et al. (2017)
Exploitative patents	We classify patents as exploratory if their combination of Cooperative Patent Classification (CPC) codes represents a novel configuration not previously observed in our data set. Conversely, patents are deemed exploitative if they reuse existing CPC code combinations, indicating an extension or refinement of established technological paths	The author calculation, Kogan et al. (2017)
Patents per employee	We follow Fich et al. (2023) to use the ratio of the number of granted patents to the number of employees to measure invention productivity	The author calculation
Sales tax per capita	The ratio of marijuana tax revenue to total population for each state in year *t*	Tax Foundation; States' Department of Taxes; The author calculation
Sales per capita	The ratio of total marijuana sales to total population for each state in year *t*	Headset; Flowhub; Marijuana Policy Project
BC law	An indicator variable that takes the value of 0 prior to the enactment of Business Combination (BC) laws, and 1 in all subsequent years, including the year of enactment	Bertrand and Mullainathan (2003), Fich et al. (2016), Karpoff and Wittry (2018), Foroughi et al. (2022
PP law	An indicator variable that takes the value of 0 prior to the enactment of Poison Pill (PP) laws, and 1 in all subsequent years, including the year of enactment	Bertrand and Mullainathan (2003), Fich et al. (2016), Karpoff and Wittry (2018), Foroughi et al. (2022)

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Variable	Definition	Sources
UD law	An indicator variable that takes the value of 0 prior to the enactment of Universal Demand (UD) laws, and 1 in all subsequent years, including the year of enactment	Bertrand and Mullainathan (2003), Fich et al. (2016), Karpoff and Wittry (2018), Foroughi et al. (2022)
CSA law	An indicator variable that takes the value of 0 prior to the enactment of Control Share Acquisition (CS) laws, and 1 in all subsequent years, including the year of enactment	Bertrand and Mullainathan (2003), Fich et al. (2016), Karpoff and Wittry (2018), Foroughi et al. (2022)
FP law	An indicator variable that takes the value of 0 prior to the enactment of Fair Price (FP) laws, and 1 in all subsequent years, including the year of enactment	Bertrand and Mullainathan (2003), Fich et al. (2016), Karpoff and Wittry (2018), Foroughi et al. (2022)

Supplementary Material

To view supplementary material for this article, please visit http://doi.org/10.1017/S0022109025102135.

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